PRACTISE AND THE WORK-CURVE

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The experimental results to be here presented concern the effect of practise in a function upon endurance in that function; but as this is a small arc of the ever-widening circle of problems in the curve of work, some review of allied literature is desirable for an appreciation of the question's setting. In some ways it is not altogether fortunate that the investigations of the curve of work have been largely dominated by the concept of fatigue. This must be mainly due to the fact that the best-known studies of the work-curve dealt with methods in which fatigue happened to be the dominant factor. Work-curve and fatigue-curve become practically synonymous, and we incline to interpret all results of this nature in terms of a paradigma of the Mosso ergogram. We know, however, that the work-curve varies essentially according to the function that works. Half an hour's series of discrete simple reaction times should show little if any fatigue loss, but a great amount of practise gain; while a much shorter period of work with the Mosso ergograph would bring the curve to a close without the superficial trace of practise gain. Other measures, as of the controlled association type, may show at first a sort of primary practise gain, after which "fatigue" gains a certain mastery; or the work may continue indefinitely without marked fluctuations.

In a paper of some four years ago, the writer discussed the comparative value of various measures of fatigue, with special reference to the tapping test.[1] The idea upon which most studies of fatigue measurement are founded, as well as that which underlay this paper, was one of some definite test which should give a criterion of general fatigue conditions or susceptibility in the individual. This conception of fatigue measurement is one closely adhered to on the educational side, in obvious consideration of the usefulness of reducing the problem of fatigue determination in school-pupils to that of a single, simple test. The possibility of doing this depends upon the correlation of fatigue phenomena in the test with

[1] *Amer. Jour. Psych.* XIX., 1908, 345-358.

those of other functions, and of them with each other, though the facts of fatigue correlation are still but imperfectly understood. There is, in fact, no special reason to suppose that a person fatiguable in the tapping test would be so in the ergograph. There may, it is true, be generalized conditions of fatiguability affecting the nervous mechanism as a whole and all that depends on it; but we have an at least equal possibility that the function of fatiguability shows as little correlation in various psychological tests as do the gross efficiences in them. One would not seek to judge a person's reaction time by his memory for musical pitch, and we have yet to show that there is certain justification for estimating the fatiguability of one function by the fatiguability of another.

It is well known that, on largely empirical grounds, there have been sought certain indirect measures of the state of fatigue. According to Yoakum, the best promise of these would seem to lie in the direction of the sphygmograph. Offner does not regard it so favorably. Aside from some practical difficulties in the way of this particular measure, remains one from which all indirect ones seem inseparable; they will not serve as measures of individual differences in fatiguability because it is not practicable to know how the relationship of the two functions differs in different individuals (or for that matter, in the same individual at different times). To put the matter more concretely, suppose that there exists a degree of correlation between certain features of the pulse-curve and the condition of fatigue. It presumably manifests a certain sort of influence which the fatigue metabolism exercises upon the circulatory system or upon the nerve-elements that affect it. The degree to which it is affected by a specific fatigue condition will depend upon its degree of responsiveness to the changes that are thus induced. The same state of fatigue could well therefore have different effects on the pulse-curve at different times or in different individuals. Ritter expresses analogous opinions, also from the educational side. The only way to get over this difficulty would seem to be to bring forward empirical evidence that the error it introduces is not sufficiently large to vitiate the practical application of results.

There is a basic difference between two methods of psychological experiment involving continued series of reactions, whether or not they be used as determinants of a work curve. We may measure the amount done at maximum speed, or we may measure in terms of errors at a moderate speed selected by the subject. Obviously, the same experiment is not suited to evaluation by either method alone, though an endeavor to

combine the two has often been made. In the first case, the endeavor is to make the experiment as free from error as possible; in the second, to ensure that it shall contain a fair percentage of errors at any rate the subject chooses. The great body of research is along the first named lines, the only extended one of the second type being that of Yoakum; but each has its advantages. The former, of which the ordinary tapping test is a type, gains in that false reactions are practically eliminated, and there is but one thing to be measured, speed; whereas in the error method the accuracy is influenced by the degree to which the 'at will' rate approximates the maximal rate, which ratio is bound to vary with the individual and with time. This is a very serious limitation where practise effects are to be compared. On the other hand, it would seem that the 'at will' rate, with scoring of false reactions, is less artificial, and corresponds more closely to the conditions of general activity. It is rather the 'at will' pressure of activity, with more or less incoherent reaction, that obtains in our daily tasks. Furthermore, as Yoakum strongly insists, such a method is less apt to be vitiated by irrelevant motor factors. It seems to the writer that the principal issue between the two methods lies neither here nor there. In the case of maximal effort we can scarcely do otherwise than interpret gradual, even though unsteady, decrease in efficiency as manifesting influences which act directly, and with some degree of continuity, upon the functions involved. That is, such factors as the actual wearing out of the mechanism; the development of fatigue toxins; or, to a less extent, reflex inhibition from fatigue sensations. In the 'at will' rate of tapping a pattern, the losses in efficiency (as given in accumulated errors) seem to be the manifestation of essentially episodic interferences. What we have here is a function whose efficiency, within ordinary limits of continuance, is not much impaired except as it is crossed at pretty definite periods by factors that produce a temporary upset in the adjustments. Such phenomena may also be seen in work at maximal rate, though while their presence in one form or another can be noted in almost any sort of continual activity, the method which Yoakum has developed from Squire is perhaps the most convenient approach to their study that has been found. The deeper causes of these interferences and the individual differences in liability to them are matters whose interest extends considerably outside their relation to the problem of fatigue. Much might be learned by the properly controlled analysis of the mental processes attendant upon such failures of adjust-

ment. Psychologically, the method is a measure of general attentional control, rather than of the efficiency in forming a series of definite associations, as the addition test or the cancellation tests. While its data are quite as important for the problem of individual differences, it seems probable that we get at least as legitimate a form of the work-curve in measures of the latter type. Of these, the historical method for the study of the work-curve is *par excellence* the addition test. Its special advantage over other directly intellectual methods is supposed to lie in the dependence upon fewer intellectual factors, and greater consequent reliability of interpretation. The Kraepelinian investigations constitute one of the most patient and thorough-going studies of a problem of its scope which occur in experimental psychology; they have been often reviewed, and, save in some special relations to the problem, it can scarcely be necessary to discuss them here.

In the published accounts it is not always clear to the reader in just what way the Kraepelinian *Rechenheft* was used. It is possible to employ it in various ways; the digits may be added cumulatively, starting afresh with each hundred, or each digit may be added simply to the next,—the latter procedure being that followed in the present instance. The way in which the experiment may be recorded also varies. The addition may be silent, the subject merely marking with a pencil the point reached at the end of each minute or other interval, indicated by a signal. This is disadvantageous since it affords no objective account of the work done. The subject may write the answers, or only the last digit of them; this is objectionable on account of the purely motor time involved.[2] The subject may indicate by a checkmark the completion of each addition, which is electrically recorded; this gives the appearance of recording the time of each addition, but really depends upon the accuracy with which the subject can estimate the process, which is more than doubtful. Von Voss, who originated this precedure, is quoted favorably and often by Yoakum; this investigator, elsewhere quite on his guard against the motor distortion of a mental work-curve, having apparently overlooked its possibility in this particular instance.

Since we must have a motor response, let it be one indicative of the work done, and most natural for the subject. The speaking of the sums would seem to be indicated, the experimenter following on a key and noting errors. If it is desired

[2] Woodworth and Wells, Association Tests, *Mon. Supp. Psych. Rev.* 57, 1911, 9-11.

to record individual processes, it can be done by recording graphically the subject's speaking into a mouthpiece; a thistle funnel will usually answer, or a stomach tube. The Kraepelinian workers have always insisted that the errors with the experiment could be disregarded; and in normal subjects it is the rule that the errors do not essentially affect gross scores. Nevertheless it is always desirable to know to what extent errors are present, and quite obligatory where there is any question of coöperation.

It is, of course, a familiar fact that the work-curve in the addition test and elsewhere represents not merely the interaction of practise and fatigue, but is the resultant of a complex of factors, some of which are favorable, others unfavorable, and most are of an evanescent character. Not only has the existence of these factors been recognized on theoretical grounds, but considerable effort has been made to analyze and determine the precise influence of each one of them upon the final work curve. The most conspicuous study from this standpoint is Specht's comparative examination of fatigue phenomena in normal individuals and in traumatic neuroses. Here, based upon a careful review of the literature, the *Pausenversuch* is specially treated with a view to separating out a specific fatigue phenomenon from all the other factors influencing the curve. Briefly stated, the contribution which the *Pausenversuch* purports to make to the simple methods of fatigue evaluation seems to be this: Given a work curve, say in the addition experiment, consisting of ten minutes uninterrupted performance, and a similar work curve under the same experimental conditions save that between the two five-minute halves a five-minute rest intervenes. One must not estimate fatiguability merely through the course of the continuous work curve, because it is subject throughout to practise gain as well as fatigue loss. In the losses of these days without the pause is expressed the extent to which fatigue overbalances practise; and if we can find some way of calculating just what this practise gain is, then we can determine how much it has offset the real phenomena of fatigue. The amount of this practise gain is supposed to be given in the relation between the performances of the second and sixth minutes of the pause experiments. Given then a performance (with the pause) eliminating the influence of fatigue, and another (without the pause) including it, the relation between the two represents the true loss by fatigue.

To the close reasoning of Specht's paper, the following criticisms are to be adduced. The calculation of fatigue and

practise from the relationship of performances at certain points in the tests rests on the assumption that these performances are essentially determined by fatigue and practise. To the extent to which these performances are influenced by such more ephemeral factors as *Antrieb, Anregung* and the like, their indications of fatigue and practise will be inaccurate. Thus an essential ratio is taken from the second and sixth minutes instead of the first and sixth, to obviate the *Anfangsantrieb* of the first minute. This is very well if one has objective means of knowing that *Anfangsantrieb* did vitiate the first minute and not the second. But if *Anfangsantrieb* unduly speeded the first minute, the performance of the second minute may be lower than otherwise, and the procedure would unduly favor those whose effort in the experiment is not sufficient to exhibit *Anfangsantrieb* or those in whom it appears more slowly. It would also be difficult to exclude, save on introspective grounds, the presence of *Anfangsantriebe* at the beginning of the second five minutes after the pause, though it is stated that they are very much less at this time. It is further possible that there is between the two halves of the experiment an *Anregungszuwachs,* " warming up " gain, that is increased under practise, as in the tapping test.[3] In the early stages of practise, the manifestations of warming-up and recovery are in the tapping test so irregular that if the same is true, in any measure, of the addition test, ratios involving the pause are very unreliable indeed. It might be said of the studies of the addition test in general that they are not sufficiently concerned with the possibility of practise effecting great changes in the relative importance of different factors in the curve. On the empirical side, the Kraepelinian analysis of the work-curve has recently undergone a searching and destructive criticism at the hands of Thorndike. According to the points brought forward in this paper, there is very little objective warrant for assigning to these factors essential import in the addition-curve at all. It would seem that the devising of these terms was an attempt to explain away certain irregularities in the work-curve in reality not subject to experimental or introspective control. The entire paper is as forceful an argument as has been presented against the application of deductive gymnastics to the study of the work-curve, and in favor of taking its phenomena so far as possible as we find them.

Some space may be reasonably devoted to these matters, since in view of the great amount of labor devoted to this

[3] *Amer. Jour. Psychol.* XX., 1908, 457-461.

analysis of the addition test, in experiments quite similar to those described below, some explanation should perhaps be given of why very little procedure of this sort is attempted here. While no one would deny a measure of existence to these different processes, they constitute too many unknown and irregular quantities in the one equation of immediate efficiency; and the writer cannot muster sufficient confidence in the quantitative analysis of these factors to feel that it is wise to attempt it. We can most nearly approach it in certain rather constant phenomena of the later stages of practise, more strikingly perhaps, in the tapping test than elsewhere. But, to quote substantially from a previous paper:

"The pragmatic significance of such measurements lies less in the determination of such abstractions as absolute fatiguability, warming-up, or impulse effects, than the objective way in which the individual responds to experimental conditions demanding the continued exercise of maximal voluntary effort. In every individual, and in every measure, the factors that determine the course of the *Arbeitscurve* have a certain way of balancing each other; and the way in which this balance varies in different individuals, and under different conditions. constitutes the essential problem of the curve of work."[4]

Attention is also called by Thorndike to the irregularity of work-curves executed by the same individual and in the same measure. It can readily be seen in the records discussed below. There are certain major features of the work-curve, including total efficiency, that regularly permit the demonstration of individual differences; in details, the factors that influence the form of the work-curve are manifestations rather of the special conditions under which the work happens to be done than of peculiarities inherent in the individual subject.

Thorndike's attitude toward the general problem is much less Procrustean than that of the Kraepelinian workers. His earlier experiments emphasize its complexity, especially the rôle played by the feelings of fatigue. Educational aspects came to the fore in a second paper, the conclusions of which pointed markedly to the feeling of fatigue as the proper point of attack, rather than the supposition of overwork. Thus we see that decreases in efficiency under continued effort, however certainly shown by well-conceived experimental method, are not necessarily the product of mental *fatigue*. The tendency of these papers was decidedly to discount this interpretation of the phenomena, and subsequent experiments have borne out this view. Some practise experiments in multiplication, re-

[4] *Cf.* Motor Retardation as a Manic Depressive Symptom, *Amer. Jour. Insanity*, 66, 1909, 5-6.

ported in 1908, were especially instructive in showing the extent to which, in minds of considerable general training, the function was still subject to practise. A positive correlation was, however, suggested between general intellectual achievement and susceptibility to practise, which assumes further significance in connection with the similar correlation of initial ability with susceptibility to practise. Experiments reported two years later showed the contrary relationship for addition in *percentile* gains; but there are objections to regarding this consideration of the gains as the correct one. In gross improvement, these results rather indicate the greater absolute gains upon the greater initial efficiency, as other, slightly different experiments have also shown.[5] An arduous series of multiplication tests regularly produced a decrease in efficiency, the more efficient individuals being the more resistant to this decrease; this is only slightly noticeable in the first periods of the present addition experiments.[6]

Wimms has published a series of experiments which deal with the relative susceptibility to fatigue and practise, though not directly with the effect of one on the other. This investigator, more under Kraepelinian influence, employed the conventional addition experiment and a more difficult multiplication task. The addition experiment showed some positive correlation of improvement and absolute amount with the multiplication tests; but the improvability in the tests is not correlated, nor does absolute amount in multiplication correspond with improvability. The present writer ventures to believe that the greater susceptibility to practise of the multiplication test may be due in no small degree to its lesser complication with motor factors. In respect to fatiguability in the two tests, as calculated through the *Pausenversuch,* some changes with practise are incidentally noted. There is no uniform tendency; in some subjects the fatigue phenomena increase with practise, in others they decrease. In general, fatiguability in the addition experiment increased with practise, contrary to the present results, while in the multiplication test the fatiguing tendency decreased. A second group of experiments dealt with mental multiplication of the same character as is described by Thorndike. The results for two-place figures show the greater improvement with the greater ability, as noted in the work of Thorndike, and the earlier addition results; but this relationship is again broken down in the harder task. The fatigue phenomena are irregularly affected by practise,—a general

[5] *Amer. Jour. Psychol.* XXIII., 75-88.
[6] See page 46.

decrease being noted in the harder, and an increase in the easier experiment, as before. The paper deals with many other relationships in improvability, fatiguability and retentivity, though the treatment is throughout a static rather than a dynamic one.

The method of equal groups,' as developed by Winch, furnishes perhaps the most immediately promising line of research in the educational problems of fatigue. One such investigation by this author, of some three years since,[7] dealing with the value of evening school work, has already been described by the writer. A somewhat more extensive study is reported by Winch in 1911 concerning the degree to which school performance varies with the hour of the school day. The phase of the question considered was that of arithmetical problems. Equal groups having been arranged on the basis of preliminary tests, in which the conditions were the same for all subjects, final tests were made in which one group worked earlier in the school-day, the other later. Four series of experiments, with as many different classes, were carried out. The final tests are in both groups apt to be better than the preliminary ones ('practise'), the general relations are expressed in the following figures:

IMPROVEMENTS OVER PRELIMINARY TESTS

	Morning group	*Afternoon group*
Infants	12.0%	0 approximately
Girls	14.6%	7.9%
Boys (1)	18.3%	11.3%
Boys (2)	7.2%	4.4%

In general averages, the group which works in the morning is uniformly better than that which works in the afternoon; the differences are nowhere great, and they tend to decrease as more mature pupils are tested. Then, as suggested in Winch's paper, other topics could be compared to see which lost the most from morning to afternoon, and least favorable times be given to subjects that would suffer least. It will be appreciated, of course, that these experiments are not so designed as to throw direct light on questions of the individual psychology of fatigue; but in their appropriateness for the solution of some concrete educational problems, they seem to constitute a rather effective rejoinder to the opponents of the 'class experiment' in this field. A problem correlative to the writer's present results, and suggested by Winch's

[7] *Jour. Educ. Psychol.* I., 1910, 13-23, 83-100.

inquiry, is that of how a given state of fatigue may affect the ability to improve by practise; *i. e.*, would Winch's afternoon work have improved by practise as much as his morning work? While at least an equally important question with the present one, it is probably but just to add that it seems many times more arduous owing mainly to the apparent lack of correlation in the practise improvability of various mental functions, and the consequent difficulty of controlling the observations. This would be necessary if we are to discover how much the fatigue state had affected the individual's capacity for practise improvement. Nothing but the practise-curve tells what the practise curve will be, though an idea of the general relation of the state of fatigue to practise improvability could, of course, be obtained from comparative practise experiments with fresh and fatigued subjects, in groups sufficiently large to offset the chance errors of selection.

The experiments from which the present results are calculated have already been described. In the addition test they consist of five-minute records, scored for every minute, with ten subjects, five men and five women, through thirty experimental days. The subjects are the same for the number-checking test; but there are considered only the results of the twenty days upon which five records were taken with each subject. The tapping test involves two subjects only, also for a practise extending over thirty days.

For the purpose of the present computations, the records of the addition test were collected into six divisions, covering five days each. In each division, from days 1-5 to days 26-30, the averages for the first minute were calculated, then those for the second minute, etc. We have then for each subject six work-curves, each made up of the averages of performances on five successive days. The question is whether the work-curves of the first division show any characteristic deviation from the later ones, which have more practise behind them.

The most salient feature of the work-curve is probably the endurance which it shows, *i. e.*, the extent to which subsequent efficiency is maintained up to or beyond the point of initial efficiency. To illustrate this for the tapping test, we adopted the procedure of stating the ratio of the initial measure to the *average* of those that immediately succeeded it. This measure was termed an 'index of fatigue,' or *f*, though there now seems to be no special reason for giving it that name except that the work-curve in the tapping test happens to be normally dominated by fatigue phenomena. The measures should be more properly regarded as an 'index of endurance,'

and is so considered in this paper, its previous denomination resulting from a one-sided outlook upon the processes involved. The measure then signifies the percentage which the average of the subsequent divisions of the work-curve is of the initial division. *If it is above* 100, *it indicates a general increase in efficiency; if below* 100, *it indicates a general decrease.*

The answer which our material furnishes to the question of how practise affects endurance in the work-curve is thus presented in a comparison of the *f*'s calculated from the curves at the different stages of practise. The complete table for the addition-test is shown in Table I.

The results are presented separately for each subject, and calculated separately for the men and women. There is an unmistakable tendency for the *f*'s to rise, more marked in the women than the men, and in fact there is a regular rise in the general average. The *f*'s are, with one exception, below 100; and it is seen that it is the initial efficiency which is normally the greatest in these records of the addition-test. The *f* for days 1-5 is the lowest of all, except in subject VI, where the second equals it, and in subject X, where it somewhat surpasses that for days 6-10. The general effect of practise upon endurance is therefore in this test a favorable one; that is, the subject does not lose so much by fatigue in the later stages of practise as in the earlier ones; or, to put the matter in another way, the initial division of the work-curve does not gain so much by practise as the subsequent ones do.

The following suggestion may be made in interpretation. The susceptibility to practise gain should decrease as the amount of practise increases. The series of the later days should not therefore be subject to so much practise gain within the single experiment as the earlier. As a matter of fact, they show more gain. The indication would seem to be that another favorable influence is present, which differs from practise in its ordinary conception, and whose effects are increased rather than lessened through continued practise. Such an influence we have already observed with the tapping test, in the 'warming-up' phenomena from series to series, much accentuated by practise. Here in the addition test, however, we seem to have a warming-up effect that is not dependent upon the pauses, as in the tapping test, but tends materially to counterbalance fatigue losses within each individual work-curve. We might conceive of it altogether in terms of increased immunity to fatigue, but for the fact that in single records the performance of the initial minute is occasionally

TABLE I

F's in the Addition Experiments

Subject....	Men Subjects							Women Subjects							Av. of Both Groups
	I	II	III	IV	V	Av.	M.V.	VI	VII	VIII	IX	X	Av.	M.V.	
Days 1– 5.....	87.6	84.7	93.7	84.1	90.9	88.2	3.3	89.7	89.4	89.6	86.2	89.9	89.0	1.1	88.6
" 6–10.....	93.1	85.1	94.2	90.5	102.1	93.0	4.2	89.7	94.5	94.3	86.8	88.4	90.7	2.9	91.9
" 11–15.....	93.1	86.1	96.3	88.0	96.0	91.9	3.9	92.0	90.4	93.9	90.1	94.4	92.2	1.6	92.0
" 16–20.....	92.1	88.3	92.3	86.3	96.6	91.1	3.1	97.5	93.6	90.6	93.7	92.1	93.5	1.7	92.3
" 21–25.....	93.8	93.5	95.8	85.4	96.1	92.9	3.2	97.8	99.4	90.2	94.9	92.3	94.9	2.9	93.9
" 26–30.....	94.0	92.1	99.2	91.6	95.9	94.6	2.4	95.3	93.4	91.6	96.2	96.3	94.6	1.8	94.6
Av. for each subject.....	92.3	88.3	95.2	87.6	96.3	91.9		93.7	93.4	91.7	91.3	92.2	92.5		92.2
M.V. for successive days.....	1.6	3.0	1.8	2.4	2.1	2.2		2.9	2.4	1.6	3.6	2.1	2.5		2.4

surpassed later, though uniformly enough to be evident in the average only in days 6-10 of subject V.

While the general trend of the records is thus towards better endurance under practise, the various subjects differ obviously in the extent to which they show this trend, and in the extent of their endurance *überhaupt*. Independently of practise, the staying power in the function is distinctly superior in subjects III and V to what it is in subjects II and IV. While the average endurance in the men and women is practically equal (91.9, 92.5), there is a pronouncedly greater variation among the men. The mean variation of these general averages for each subject is 3.2 for the men; for the women, .9. If we consider the mean variations in the two groups of men and women subjects separately for each five-day period, we find them consistently smaller in the women, the average being 3.3 for the men, for the women 2.0. This is a rather more marked difference than the similar one observed in the work-curve of the tapping test. But as was brought out in this previous paper,[8] it is uncertain that this relationship would be maintained in work-curves of longer duration. It may also be noted that the progressive improvement of endurance is somewhat more regular in the women than in the men.

With the number-checking test the conditions are different. The work, which consists of executing in succession five of the number-checking blanks described by Woodworth and Wells,[9] is not continuous, but interrupted on account of the times necessary to substitute a fresh blank. The length of these pauses could not have varied greatly, though it was not so precisely controlled as it would have been had the experiments been undertaken with the present calculations in view. For reasons already mentioned, only twenty days, *i. e.*, four groups of five experiments each, are available in this experiment, and they do not represent the beginning of special practise in the test. When calculated in the same way as the addition test, the results appear as in Table II.

The main differences between these figures and those of the addition test are that the *f*'s are, with one exception, well above 100, indicating increased efficiency over the initial work period, instead of decrease (in view of the pauses, we should probably expect it), and that there is little if any progressive alteration of this increase, with practise. Some individuals, as subjects VI and VII, show the same phenomena as were regularly observed in the addition test; but others, as I and IV, show rather the reverse of it. The salient fact is that the

[8] *Amer. Jour. Psych.* XX, 359.
[9] *Loc. cit.* 24-29.

TABLE II

F's in the Number Checking Test

Subject....	Men Subjects							Women Subjects							Av. of Both Groups
	I	II	III	IV	V	Av.	M.V.	VI	VII	VIII	IX	X	Av.	M.V.	
Days 11–15[1].....	103.1	103.9	108.8	110.6	107.3	106.7	2.6	103.5	101.8	109.4	108.6	103.1	105.3	3.0	106.0
" 16–20.....	107.9	98.5	114.4	117.6	106.4	108.9	5.6	110.9	103.8	108.7	103.9	102.6	106.0	3.1	107.5
" 21–25.....	103.0	101.4	108.2	110.9	107.5	106.2	3.2	10.9	105.3	103.4	111.0	105.0	107.1	3.1	106.6
" 26–30.....	103.6	109.4	106.3	109.5	103.4	106.4	2.4	108.3	108.0	105.7	110.0	105.5	107.5	1.5	107.0
Av. for each subject.....	104.4	102.6	109.4	112.1	106.1	107.0		108.4	104.7	106.8	108.4	104.0	106.4		106.7
M.V. of this average.....	1.7	3.1	2.5	2.6	1.5	2.3		2.5	1.9	2.2	2.2	1.2	2.0		2.1

[1] The records of days 1–10 include but one test on each day, and are not available for these calculations. Cf. Am. Jour. of Psych., XXIII, p. 77.

subsequent performances uniformly show marked improvement over the initial ones of the several experimental days. This improvement is more analogous to that noted as 'warming up' in the tapping test, but it is not regularly accentuated by practise. As there, the improvement is evanescent, being regularly lost from day to day; so that the best series may be some days' practise ahead of the initial one. Kraepelin's pupils might speak of *Anregung* or of marked *Uebungsfähigkeit bei geringer Uebungsfestigkeit*. The argument against its being in the nature of practise is that it should then become less marked with increasing practise, which it shows no general tendency to do. It is rather a process which repeats itself *de novo* from day to day, on the basis of the true practise gain.

But while there is slight progressive change of the endurance in this test, made in this manner, there is marked individual variation in the extent to which different subjects manifest the 'warming up' effect. As before, there is not much difference between the men and the women subjects, as groups; but the extremes are in each case found in the records of the men. The *f*'s of the subjects could be arranged in a fairly reliable order, which, it may be remarked, would differ rather widely from that in the addition test above. This would indicate that *Anregbarkeit* is not a general property of the individual, but might be great in one function and small in another, as was previously found to be the case with susceptibility to practise improvement.

For an applied psychology, at least, the interest of such data does not end with showing the presence or absence of a tendency that is pervasive. A measure of central tendency is of at least equal significance as a fixed point by which to measure the relative distances of individual variations from it and from each other. If, in respect to the effect of practise on the work-curve, some persons show one tendency and some an opposite tendency, it is clear that no universal rule for the effect of practise on the work-curve can be laid down; but it is also necessary to know whether the presence of one tendency or another, in definite degree, is a fundamental property of the individual. Different persons may vary widely about their group average; but if their differences are characteristic, they are much more important psychologically than a central tendency which may be fairly reliable for the group, but about which the same individual may vary now in one direction, now in another. Indeed, the greater the mean variation of a group average, the greater the possibility of genuine and significant individual differences about it.

That such individual differences exist in the present results has already been mentioned; *e. g.*, the inferior endurance of II and IV in the addition test, and the superior gains of III and IV in the number-checking test. Given such differences, it remains to be seen whether this is the limit of their significance. It is a general experience of experimental psychology that the interpretation of its present tests is confined rather closely to the special function of each test itself. There is little in the present results to change this view. The favorable positions in the addition test do not correspond well with those in the number-checking test. In the addition test, the Pearson coefficient of the gross efficiency for days 1-5 and the endurance of those days, is +.28, and even this relationship disappears in days 26-30, where it becomes .02. The number-checking test shows no significant relationship between gross efficiency and response to the conditions of *Anregung*. These results therefore confirm the results of previous investigations of the subject, not only that the proper study of the work-curve is the work-curve; but also such a work-curve as shall approximate in the closest possible way to the conditions under which the more vital activities of the subjects take place.

Another paper has presented in detail the effects of practise on the work-curve in the tapping test. They need be summarized only for comparative purposes here. In the main question, the results of the two subjects disagreed, subject I showing poorer endurance under practise, subject II better endurance. The practise improvement in this function is but a small fraction of that in the tests described above. These experiments thus afforded no reason to suppose a general effect of practise on the form of the single work curve, in the sense of that in the addition test. On the other hand, distinct evidence appeared in both subjects that practise improved the effect of the pauses, and that the principal way in which it improved them was by giving immunity to fatigue loss. This would again argue for a separation of the gains of *Anregung* and of *Uebung*. In all experiments we seem to see some kind of favorable influence on the work-curve,—be it one of the immunizing it to fatigue, or raising its general level,—whose effect is but exceptionally lessened by daily practise within ordinary limits, being, on the contrary, rather increased by it. It would seem then that we might best bring together certain favorable effects of practise on the form of the work-curve under the conception of an increased response to *Anregung,* recognizing, however, that this response may show itself (1) in better endurance in the single work-curve (the

addition test); (2) in an increasingly favorable effect of the pause (the tapping test). In the number-checking test, this effect does not seem to be general; the phenomena of warming up are present, but are slightly and variously affected, in some persons favorably, in some otherwise, under the influence of practise.

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